EXHIBIT J

**To Declaration of Micah West in Support of
Motion for Preliminary Injunction
& Motion for Class Certification**

Distortion of Justice: How the Inability to Pay Bail Affects Case Outcomes

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Abstract

This paper uses a natural experiment to analyze whether incarceration during the pretrial period affects case outcomes. In Philadelphia, defendants randomly receive bail magistrates who differ widely in their propensity to set bail at affordable levels. Using magistrate leniency as an instrument, I find that pretrial detention leads to a 13% increase in the likelihood of being convicted, an effect largely explained by an increase in guilty pleas among defendants who otherwise would have been acquitted or had their charges dropped. Pretrial detention also leads to a 41% increase in the amount of non-bail court fees owed and a 42% increase in the length of the incarceration sentence. I find large gaps in the pretrial detention rate across the race and neighborhood wealth levels of defendants, partially accounted for by differences in the likelihood of posting monetary bail. If black defendants posted bail at the same rate as non-black defendants, their average detention rate would decrease by 6 percentage points, or half of the entire race gap in detention.

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I have had the 'you can wait it out or take the deal and get out' conversation with way too many clients. -a public defender, Philadelphia

1   Introduction

There are currently 467,000 people awaiting trial in jail in the United States (Minton and Zeng, 2015). In fact, there are more people in jail awaiting trial than are incarcerated due to a drug sentence.[1] This number is particularly striking considering that our criminal justice system is founded on a presumption of innocence, where, at least in theory, "liberty is the norm, and detention prior to trial or without trial is the carefully limited exception."[2]

According to Bureau of Justice Statistics, five out of six people detained before trial on a felony charge are held on money bail (Cohen and Reaves, 2007). Some of these defendants are facing very serious charges, and accordingly have very high bail. But many have bail set at amounts that would be affordable for the middle or upper-middle class but are simply beyond the reach of the poor. In Philadelphia, the site of this study, more than half of pretrial detainees would be able to secure their release by paying a deposit of $1000 or less, most of which would be reimbursed if they appear at all court dates. Many defendants remain incarcerated even at extremely low amounts of bail, where the deposit necessary to secure release is only $50 or $100. Nor are the charges faced by many pretrial detainees particularly serious: 60% of those held for more than three days were charged with non-violent crimes and 28% were charged only with a misdemeanor.

It has long been postulated that pretrial detention increases the likelihood of conviction and the severity of sentences. A defendant may plead guilty to get out of jail, or accept an overly punitive plea deal because detention impaired her ability to gather evidence or meet with her lawyer. Adjustment to life in jail, combined with the potential loss of employment or housing, may reduce a defendant's incentives to fight the charges. While prior research has shown a correlation between pretrial detention and unfavorable case outcomes, it did not show that the relationship was causal.[3] Those detained differ from those released in ways that are both observable and unobservable to the researcher; detainees tend to be facing more serious charges and have longer criminal histories, and they may also have stronger evidence against them. They are expected to have worse case outcomes regardless of detention status. Isolating the

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[1]The number of state and federal prisoners whose most serious offense was drug-related is found in Minton and Zeng (2015). The most recent information on the percentage of convicted jail inmates with a drug sentence is from James (2004).

[2]Chief Justice Rehnquist in United States v. Salerno, 1987

[3]See Ares et al. (1963); Rankin (1964); Goldkamp (1980); Williams (2003); Phillips (2007, 2008); Tartaro and Sedelmaier (2009); Sacks and Ackerman (2012); Lowenkamp et al. (2013); Oleson et al. (2014)

causal effect of pretrial detention requires an experimental research design.[4]

In this paper I present some of the first quasi-experimental evidence that pretrial detention increases the likelihood of being convicted, pleading guilty, receiving lengthy incarceration sentences, and being required to pay hundreds of dollars in court fees. The research design takes advantage of the fact that defendants randomly receive bail magistrates who vary widely in their propensity to set bail at affordable levels. Those who receive a strict magistrate are statistically identical to those who receive a more lenient magistrate except in their likelihood of being detained pretrial. If those who receive a strict magistrate are also more likely to be convicted or receive unfavorable sentences we can infer that this is due to differences in detention rates and not some other unseen difference in defendant or case characteristics.

The data used in this analysis covers all criminal cases originated in Philadelphia between September 2006 and February 2013, with a total sample size of 331,971 cases. The rotating work schedule of the bail magistrates creates random variation in which magistrate is on duty; each magistrate works an equal number of night shifts, weekend shifts, etc. The duties of the bail magistrate are very limited and there are few plausible alternative channels through which they could affect case outcomes. After the bail hearing, the magistrates do not interact with the defendant or make any other decisions related to her case, nor does the schedule of the magistrates align with that of the judges or any other actors in the criminal justice proceedings. The institutional features of Philadelphia's pretrial process provide a particularly clean natural experiment with which to estimate the impacts of pretrial detention.

For each defendant, I build an instrument for pretrial detention that consists of the average detention rates of *other* defendants who had bail set by the same magistrate. Using this measure of magistrate leniency as an instrument, I estimate that pretrial detention leads to a 6.2 percentage point increase in the likelihood of being convicted on at least one charge, over a mean 49% conviction rate. The effect on conviction (being found guilty either through plea or at trial) is largely explained by a 4.7 percentage point increase in the likelihood of pleading guilty among those who would otherwise have been acquitted, diverted, or had their charges dropped. Those detained will be liable for $129 more in non-bail court fees (a 41% increase over the mean), and will be sentenced to an additional 124 days of incarceration (a 42% increase over the mean).

The adverse effect that pretrial detention has on case outcomes deepens concerns about socio-economic disparities in pretrial detention, particularly since detention sta-

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[4]Ares et al. (1963) used an experimental method to look at the impact of pretrial detention on case outcomes but did not present the results in a manner that allows for causal interpretation. Goldkamp (1980) and Abrams and Rohlfs (2011) used a randomized experiment to look at how the bail amount affects crime, flight, and the likelihood of posting bail, but did not evaluate the impacts on case outcomes.

tus depends partly on the ability to post bail.[5] I find that black defendants are 40% more likely to be detained than non-black defendants, and defendants from low-income neighborhoods are 17% more likely to be detained than those from wealthier neighborhoods. Part of this gap is due to differences in the charges the groups are facing, and therefore differences in the amount of bail set. Another part is due to the fact that black defendants and those from low-income neighborhoods are less likely to post a given amount of bail. For example, only 46% of black defendants with bail set at $5000 post bail within three days, compared to 56% of non-black defendants. I find that if black defendants posted bail at the same rate as non-blacks, and those from low-income neighborhoods posted bail at the same rates as those from wealthier neighborhoods, the gaps in detention rates would be cut in half. Median income for black households is less than 2/3 that of white households in Philadelphia (Ingram, 2007). The lower rates of posting bail for black defendants and those from poor neighborhoods is likely to be at least partially explained by differences in wealth, income and access to credit.

The results of this paper speak to several important legal and policy issues. First, the downstream criminal justice consequences of pretrial detention underline the importance of making sure that defendants aren't being detained just because they are poor. This could be achieved by reducing the use of money bail, or implementing procedures to ensure that the bail amount is proportional to defendants' financial resources.[6] Risk assessment tools can be helpful in determining which defendants can be released under minimal conditions.[7] Second, the results of this paper bolster the argument that the bail hearing is a 'critical stage'. Since defendants have a constitutional right to counsel at all 'critical stages' in a criminal proceeding, this would suggest that defendants should have the right to counsel at bail hearings. More generally, this research shows that the bail hearing is important, since the decisions made at the bail hearing have serious downstream consequences. Currently, bail hearings in Philadelphia – as in many jurisdictions – last only about a minute, occur over videoconference, and do not include legal representation for the defendants. Taking more time and care during the bail hearing, including providing increased training and guidance to the bail magistrates, would likely improve the decisions made at this important juncture.

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[5]A literature review of racial disparities in bail finds mixed results (Free, 2004). One interesting prior paper shows that bail bondsmen charge lower rates for blacks than whites, suggesting that blacks pose a lower risk than whites at the same bail amount (Ayres and Waldfogel, 1994).

[6]A group of civil rights lawsuits have recently argued that bail which does not take ability to pay into account is in violation of Equal Protection and due process. Citing a series of Supreme Court rulings in the wake of Griffin v. Illinois (1956), these cases have had initial success in the district courts. See Section 8 for more details.

[7]Caution is warranted, however, since risk assessment tools that include race and income proxies, like zip code, may perpetuate socio-economic disparities in detention.

In the months since this paper was first circulated, several other papers, developed in parallel and also evaluating the impact that pretrial detention has on case outcomes, have been released. Gupta et al. (2016), Heaton et al. (2016), Dobbie et al. (2016) and Leslie and Pope (2016) have shown that pretrial detention is associated with unfavorable case outcomes in New York City, Houston, Miami, Philadelphia, and Pittsburg. My paper differs from each in various ways; one difference is the focus on the socio-economic disparities induced by money bail.

In Section 2 I give a brief overview of the pretrial process, in Section 3 I describe the natural experiment, and in Section 4 I discuss the data and provide descriptive statistics and graphs. Section 5 discusses the empirical strategy and provides evidence that magistrate assignment is as-good-as-random. Section 6 presents the results for the full sample and provides several robustness checks. Section 7 shows results for various subgroups. Section 8 analyzes socio-economic disparities in detention rates and discusses some of the implication of the results. Section 9 concludes.

2   The pretrial process

Pretrial detention is the act of keeping a defendant confined during the period between arrest and disposition for the purposes of ensuring their appearance in court and/or preventing them from committing another crime. The vast majority of jurisdictions use a money bail system to govern whether or not a defendant is detained (PJI, 2009). In such a system a judge or a magistrate determines the amount of the bail required for release and the defendant is only released if she pays that amount and agrees to certain behavioral conditions. In some cases the defendant will be released without having to pay anything, in others (usually only the most serious cases) she will be denied bail and must remain detained. While the defendant is liable for the full amount of the bail bond if she fails to appear at court or commits another crime during the pretrial period, she usually does not need to pay the full amount in order to secure release. In many jurisdictions she will borrow this sum from a bail bondsman, who charges a fee and holds cash or valuables as collateral (Cohen and Reaves, 2007). In some jurisdictions, Philadelphia included, the courts act as a bail bondsman and will release the defendant after the payment of a deposit.

Bail hearings are generally quite brief – in Philadelphia most last only a minute or two – and often do not have any lawyers present.[8] After the bail hearing there are a series of pretrial court appearances that defendants must attend. Although the

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[8]PJI (2009) shows 40% of respondent districts do not have defense attorneys at bail hearings. While there is no systematic survey of the length of bail hearing, they are reported to be very short in many jurisdictions: three minutes long in North Dakota (VandeWalle, 2013), less than two minutes in Cook County (Staff, 2016) and only a couple minutes long in Harris County (Heaton et al., 2016).

exact procedure varies across jurisdictions these usually include at least an arraignment (where formal charges are filed) and some sort of preliminary hearing or pretrial conference (where the case is discussed and plea deals can be negotiated). Plea bargaining usually begins around the time of arraignment and can continue throughout the criminal proceedings. In some jurisdictions, like New York City, the arraignment happens simultaneous to the bail hearing and it is not uncommon to strike a plea deal at this first appearance (Barry et al., 2012). In other jurisdictions, such as New Orleans, arraignments for felony defendants often do not happen until four months after the bail hearing and a defendant who is unable to make bail must wait until then to file a plea.[9] In Philadelphia, arraignments usually happen within a month of the bail hearing.

Plea negotiation is a process in which the defendant receives reduced charges or shorter sentences in return for pleading guilty and waiving her right to a trial. Since defendants often face severe sentences if found guilty at trial, the incentives to plead are strong. It is estimated that 90-95% of felony convictions are reached through a plea deal (Devers, 2011). Philadelphia differs from many other jurisdictions in its wide use of bench trials on felony cases. Since sentencing tends to be more lenient in bench trials than jury trials, this reduces the incentive to plead guilty.[10] Only about 78% of felony convictions are reached through plea in Philadelphia. Trial by jury is not constitutionally required if the maximum incarceration sentence is less than six months, and the use of bench trials for misdemeanors, as is the custom in Philadelphia, is more common across jurisdictions.

There are a number of reasons why a detained defendant might be more likely to be convicted, or receive a more punitive sentence. Any plea deal that involves immediate release from jail would be very tempting, even if the deal involved onerous probation requirements, heavy fines, and negative impacts on future labor market prospects or access to public benefits (Bibas, 2004). It may be that since some of the disruptions of incarceration have already occurred – loss of job/housing, the initial adjustment to life behind bars – the incentives to fight the charges are lower. Jail may affect optimism about the likelihood of winning the case, or, by changing the reference point, may affect risk preferences in such a way that the certainty of a plea deal seems preferable to the gamble of a trial. Detention also impairs the ability to gather exculpatory evidence, makes confidential communication with attorneys more difficult, and limits opportunities to impress the judge with gestures of remorse or improvement (taking an anger

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[9]Based on discussions with former New Orleans Parish defenders.

[10]In Philadelphia, a bench trial is the default for all but the most serious felonies. The right to a jury trial can be asserted upon request, but this is uncommon. While there is no formal mechanism that ensures that a bench trial will lead to better outcomes for the defendant than a jury trial, all defense attorneys interviewed assured me that this was the case.

management course, entering rehab, etc.) (Goldkamp, 1980). Detained defendants may attend pretrial court appearances in handcuffs and/or prison garb, creating superficial impressions of criminality. Furthermore, if a defendant must await trial behind bars he may be reluctant to employ legal strategies that involve delay. While a released defendant may file continuances in the hopes that the prosecution's witnesses will fail to appear, memories will blur, or charges eventually get dropped, a detained defendant pays a much steeper price for such a strategy. More nefariously, those detained have less opportunity to coerce witnesses, destroy evidence or otherwise impede the investigation (Allen and Laudan, 2008).

These different mechanisms through which pretrial detention could affect case outcomes are likely to vary in importance by defendant and according to the local characteristics of criminal procedure. While there is little reason to believe that the results shown in this paper are unique to Philadelphia, the magnitude of the effects may differ across jurisdictions.

3   The natural experiment

Immediately after arrest, arrestees are brought to one of seven police stations around the city. There, the arrestee will be interviewed via videoconference by Pretrial Services. Pretrial Services collects information about various risk factors as well as financial information to determine eligibility for public defense. Using risk factors and the current charge, Pretrial Services will determine the arrestee's place in a 4 by 10 grid of bail recommendations. Although these bail guidelines suggest a wide range of appropriate bail, they are only followed about 50% of the time (Shubik-Richards and Stemen, 2010). Once Pretrial Services has entered the bail recommendation and the financial information into the arrest report the arrestee is ready for her bail hearing.

Once every four hours the magistrate will hold bail hearings (in Philadelphia these are called Preliminary Arraignments) for all arrestees who are ready. The bail hearing will be conducted over videoconference by the magistrate, with a representative from the district attorney's office, a representative from the Defender Association of Philadelphia (the local public defender), and a clerk also present. In general, none are attorneys. The magistrate makes the bail determination on the basis of information in the arrest report, the pretrial interview, criminal history, bail guidelines, and advocacy from the district attorney and public defender representatives.

There are four things that happen during the bail hearing: the magistrate will read the charges to the arrestee, inform her of her next court appearance, determine whether the arrestee will be granted a court-appointed defense attorney, and set the bail amount. The first two activities are formalities that ensure the defendant is aware

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of what she is being charged with and where her next court date is. Eligibility for public defense is determined by income. If the defendant is deemed eligible, she will be assigned either to the Defender Association, or to a private attorney who has been approved to accept court appointments by the City of Philadelphia. The default is to appoint the Defender Association; if procedural rules require the court to appoint an attorney outside of the Defender Association the magistrate's clerk will appoint the attorney at the top of a rotating list of eligible attorneys known as a 'wheel'.[11]

A typical bail hearing lasts only a minute or two and the magistrate has broad authority to set bail as she sees fit.[12] Bail decisions fall into three categories: release with no payment required, cash bail or no bail.[13] Those with cash bail will be required to pay a 10% deposit on the total bail amount in order to be released. After disposition, and assuming that the behavioral conditions of the pretrial period were met, 70% of this deposit will be returned. The City of Philadelphia retains 30% of the deposit, even if charges get dropped or the defendant is acquitted on all charges. Those who do not have the 10% deposit in cash can borrow this amount from a commercial bail bondsman, who will accept cars, houses, jewelry and other forms of collateral for their loan. If the defendant's arrest occurred while she is already on probation or parole, her probation officer may choose to file a detainer. If a detainer is filed she may not bail out until a judge removes the detainer.[14]

The research design uses variation in the propensity of the magistrates to assign affordable bail as an instrument for detention status. The validity of the instrument rests on several factors, including that the magistrate received is essentially random and that the instrument not affect outcomes through a channel other than pretrial detention. The following details help ease concerns along these lines.

Philadelphia employs six Arraignment Court Magistrates at a time, and one of the six will be on duty 24 hours a day, 7 days a week, including holidays. Each day is composed of three work shifts: graveyard (11:30 pm-7:30 am), morning (7:30 am-3:30 pm) and evening (3:30 pm-11:30 pm). Each magistrate will work for five days on a

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[11]If there are multiple codefendants, such that representing all of them would pose a conflict of interest, one defendant will be randomly selected to be served by the Defender Association and the others will receive a court-appointed attorney. For opaque historical reasons, four out of five defendants charged with murder will be represented by court-appointed attorneys and the fifth will be represented by the homicide division of the Defender Association (Anderson and Heaton, 2012). This decision is made by the order in which defendants are entered into the data system and the court-appointed attorney is chosen by a Municipal Court judge, not a magistrate.

[12]If either the defense or the prosecution is unhappy with the decision they can make an appeal to a judge immediately after the bail hearing. However the bar is high for overturning the original bail decision so this is not very common.

[13]Holding a defendant without bail is uncommon, although bail is sometimes set at prohibitively high rates.

[14]The detainer hearing usually happens within a week of arrest. Detainer cases are evenly distributed across magistrates and should not bias the results.

particular shift, take five days off, then do five days on the next shift, five days off, and so forth. For example, a magistrate may work the graveyard shift from January 1st to January 5th, have January 6th-10th off, then work the morning shift from January 11th-15th, have the 16th-20th off, do the evening shift from January 21st-25th, take the next five days off, and then start the cycle all over again.

This rotation relieves concerns that certain magistrates set higher bail because they work during shifts that see higher-risk defendants. Over time, each magistrate will be scheduled to work a balanced number of weekends, graveyard shifts, and so forth. However the magistrates do not always work their appointed shifts; in fact, about 20% of the time there is a substitute (usually one of the other magistrates). To avoid potential confounds I instrument with the magistrate who was scheduled to work instead of the magistrate who actually worked. Furthermore, arrestees do not have latitude to strategically postpone their bail hearing to receive a more lenient magistrate. The process from arrest to bail hearing has been described as a conveyor belt: on average the time from arrest to the bail hearing is 17 hours and defendants are seen as soon as Pretrial Services notifies the Arraignment Court that they are ready (Clark et al., 2011). Thus the magistrate received by each defendant is essentially random, at least in that the sample of defendants who are seen by each magistrate should be statistically identical. I confirm this empirically in Section 5.

Since the duties of the bail magistrate are so limited, there are few channels outside of the setting of bail through which the magistrate could affect outcomes. One concern would be a correlation between the schedules of the magistrates and the likelihood of receiving a particular judge, prosecutor or defense attorney later on in the criminal proceedings. However the peculiar schedule of the magistrates does not align with the schedule of any other actors in the criminal justice system. For one, this is because the other courts are not open on weekends. This is also because Philadelphia predominantly operates on a horizontal system, meaning that a different prosecutor handles each different stage of the criminal proceedings. Likewise, if the defendant is represented by the Defender Association ($\sim$60% of the sample), she will have a different defense attorney at each stage.[15] While attorneys often rotate duties, their rotations are based on a Monday-Friday work week and not the 'five days on, five days off' schedule of the magistrates.

Eligibility for public defense is another potential channel through which the magistrate could affect outcomes; 75% of the sample has a public defender at the time of disposition. However there is no correlation between the leniency of the bail magistrate and having a public defender. This can be seen in Figure 1a, where the x and

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[15]The most serious cases are not handled horizontally, however the choice of attorney to handle these cases has nothing to do with the magistrate.

y axes show residuals from regressions of conviction and having a public defender (respectively) on controls for the time and season of the bail hearing. The time controls account for the fact that certain magistrates do not work through the entire time period of my data, and each dot represents the average per magistrate. There is no visible correlation between the likelihood of receiving a lenient magistrate and the likelihood of having a public defender. (Nor is there any statistically significant relationship between the two in a regression.) In Section 6 I show that controlling for whether or not the defendant is represented by a public defender has no meaningful effect on the main results.

The only other condition of release that the magistrates are responsible for is determining whether the defendant must phone in periodically with Pretrial Services. As of 2009, approximately 9% of defendants were required to call into pretrial services either once or twice a week as a part of their condition of release (Clark et al., 2011). These phone calls are made to an interactive voice-response system, there is no therapeutic element involved. Those who violate the call-in requirement do so with impunity: no violation notice is sent to the court, nor are any sanctions applied (Clark et al., 2011). It is unlikely that these calls will have more than a minor effect on case outcomes. In the Appendix I provide further evidence that the main results are robust to inclusion of controls for the telephone call-in requirement.

More invasive conditions of release are available to judges later in the criminal proceedings, but not to the magistrate who makes the initial bail assignment. These include electronic monitoring, drug testing, substance abuse counseling, in-person meetings with pretrial services or house arrest. As of 2009, only about 1% of arrestees were assigned to any of these conditions (Clark et al., 2011). The schedules of the judges who assign these conditions of release do not correlate with the rotating schedule of magistrates.

4   Data and descriptive statistics

The data for this analysis comes from the court records of the Pennsylvania Unified Judicial System. PDF files of case dockets and court summaries were acquired by web-scraping public records; these were converted into data suitable for statistical analysis by text-parsing. The data covers all Philadelphia arrests in which charges were filed between September 13, 2006 and February 18, 2013. Before September 13, 2006, Philadelphia used a different data management system and the data from that time period is of much lower quality. I do not look at cases which began after February 18, 2013 both because I wanted to leave ample time for all cases to resolve and because one of the magistrates was replaced by a new one on that date.

Each observation in my data set refers to a particular criminal case. A case can have multiple charges and a defendant can have multiple cases. Information about the bail amount, the magistrate, the bail hearing, and the charges at the time of the bail hearing comes from the Municipal Court (lower court) dockets. Information about court fees and whether the defendant is held pretrial on a detainer can be found in the Municipal Court dockets as well as the Court of Common Pleas (felony court) dockets. In addition, each defendant has a Court Summary Report, which summarizes the outcomes of each criminal case in which charges were filed in Pennsylvania. This provides both criminal history and recidivism information, as well as other general descriptors of each case (outcomes, sentencing, attorneys, dates of arrest/disposition etc.). Average gross income for each zip code in 2010 was acquired from IRS.gov.[16]

A few constraints of the data should be noted. First, criminal history and recidivism is only available for crimes committed within Pennsylvania. Of these, I have the full range of past crimes, and all future crimes as of December, 2015. Second, the data does not allow me to distinguish between concurrent and consecutive incarceration sentences. The definition of the length of incarceration that is used in this paper is the longest sentence received. Finally, a small subset of the data got lost in the web-scraping process. I am missing key data sources for about 0.33% of the sample (about 1000 cases), these have been dropped. Since these missing variables are due to technical errors in the download, they should not result in any systematic selection of cases and are not expected to affect the results. The final sample consists of 331,971 cases.

Figure 1b shows a histogram of the number of days defendants are detained before disposition, conditional on being detained more than three days and less than 600 days. The left tail of the distribution is omitted since the primary definition of 'detainees' used in this paper is being unable to make bail within three days; the long right hand tail of the distribution is omitted for visual simplicity. The median number of days detained for those who are unable to make bail within three days is 78, the mean is 146.

Summary statistics for the released group, the detained group, and the whole sample are shown in Table 1. Defendants are predominantly male, with an average age of 32. The race data available is not highly nuanced: 57% are labeled "black", 28% are labeled "white" and the rest are either missing or labeled as "unknown". Those detained tend to have longer criminal histories and are facing more serious charges than those released. 11% of detainees are charged with a violent crime such as robbery, aggravated assault, murder, rape or burglary. It should be noted, however, that 28% of the detained sample are only facing misdemeanor charges.[17]

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[16]https://www.irs.gov/uac/soi-tax-stats-individual-income-tax-statistics-zip-code-data-soi

[17]The offense information used in this paper is taken from the charge at the time of the bail hearing. Many of those who were originally charged with felonies subsequently had the felony charge downgraded to

Almost half the sample have their charges dropped, dismissed, or are placed in some sort of diversion program.[18] Almost everyone else was convicted, through plea or at trial, on at least one charge. 90% of cases resolved at trial result in convictions, suggesting that prosecutors will not bring a case to trial if they don't believe they have a strong chance of winning. If a detained defendant pleads quickly to avoid more time waiting in jail, she may be pleading guilty on a case that otherwise would not have proceeded to court.

One third of the sample is released without being required to pay bail and an additional 26% are able to pay their way out within three days of the bail hearing. Figure 1c shows the distribution of bail amounts for all defendants. The median amount of bail for the detained group is $10,000. About 10% of the sample has bail set at an amount greater than $0 but less than or equal to $2000. Among this low-bail sample – 77% of whom are charged only with misdemeanors – the average number of days detained pretrial is 28, and 40% are detained for at least four days. This group would need to pay a deposit of $200 or less to secure their freedom.

Figure 1d shows the percentage detained and released at various levels of bail. This sample is limited to defendants who do not have a detainer placed on them – in other words, these defendants would be free to leave if they posted bail. Almost 40% of defendants with bail set at $500 do not post bail within three days of the bail hearing. These defendants would only need to post a deposit of $50 in order to secure release. While a percentage may prefer to stay in jail, it is reasonable to infer that many would post bail if they could afford it. As of 2008, Philadelphia's jails housed 44% more inmates than they were designed to, and 20% of inmates were living in "triple cells" (three inmates in a cell designed for one or two people).[19] "Lock-downs" and restrictions on movement are common, and despite the heat and humidity which characterize Philadelphia's summers, many buildings lacked air conditioning.

5   Empirical strategy

Instrumenting for sentencing outcomes using varying propensities of randomly assigned or rotating judges is a popular method of identifying causal effects in criminal justice (Kling, 2006; Aizer and Doyle, 2009; Loeffler, 2013; DiTella and Schargrodsky, 2013; Mueller-Smith, 2015). My empirical specification follows in that tradition, most closely resembling that of Mueller-Smith (2015) and a specification used in a robustness

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a misdemeanor.

[18]Diversion programs are designed for those with low level misdemeanor charges; if the defendant agrees to requirements such as paying restitution to victims, entering rehab, or performing community service, they are generally able to avoid a formal adjudication of guilt and a criminal record.

[19]From Williams v. City of Philadelphia, 2008

test in Aizer and Doyle (2009). I use a jackknife (leave-one-out) instrumental variables method, allowing the preferences of the magistrate to vary across three time periods and according to the offense, criminal history, race and gender of the defendant. The first stage of this specification is shown in Equation 1 where a dummy for pretrial detention in case i ($Detention_i$) is regressed on the magistrate dummy ($Magistrate_i$) interacted with a subset of covariates (Cov_i^{sub}) and with indicators for three time periods (T_i), as divided by February 23, 2009 and February 23, 2011.[20] Other offense, criminal history, and demographic controls are included in X_i, and controls for the time and date of the bail hearing are included in $Time_i$.[21][22] The instrument for pretrial detention for the defendant in case i is thus the average detention rate of all other individuals with a similar offense, criminal history, race and gender who had their bail set by the same magistrate during a two year period.

$$Detention_i = \alpha_1 + Magistrate_i * T_i * \omega_1 + Magistrate_i * Cov_i^{sub} \\ * \phi_1 + Cov_i^{sub} * T_i * \delta_1 + X_i * \gamma_1 + Time_i * \psi_1 + e_i \tag{1}$$

The second stage of the two stage least squares regression is shown in Equation 2 where $Case\_Outcome_i$ represents a variety of case outcomes, $\widehat{Detention}_i$ is the fitted value from the jackknifed first stage, and Cov_i^{sub}, X_i, T_i and $Time_i$ are as described above.

$$Case\_Outcome_i = \alpha_2 + \widehat{Detention}_i * \beta_2 + Cov_i^{sub} * T_i * \delta_2 + X_i * \gamma_2 + \\ Time_i * \psi_2 + \epsilon_i \tag{2}$$

Each magistrate sees about 17,000 cases during a two year period. Since the interaction effects are additive, the instrument for each case will be estimated off of many

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[20]Cov^{sub} consists of the following variables: dummies for the 17 most common offenses (murder, robbery, aggravated assault, burglary, theft, shoplifting, simple assault, drug possession, drug sale, drug purchase, marijuana possession, 2nd degree felony firearm possession, 3rd degree felony firearm possession, vandalism, prostitution, first offense DUI, motor vehicle theft), a dummy for being labled black, a dummy for being female, the number of prior cases, the number of prior violent crimes, a dummy for having at least one prior and a dummy for having a detainer.

[21]X_i includes controls for age, age squared, age cubed, the number of prior felony cases, prior cases where the defendant was found guilty of at least one charge, dummies for having at least one prior case, having at least three prior cases, awaiting trial on another charge, and having a prior arrest within five years of the bail hearing. Offense variables include dummies for having a charge in the following category: rape, possession of stolen property, second offense DUI, resisting arrest, stalking, indecent assault, arson, solicitation of prostitutes, disorderly conduct, pedophilia, intimidation of witnesses, accident due to negligence, false reports to a police officer, fleeing an officer, and reckless endangerment. Additional offense controls include dummies for being charged with a first, second or third degree felony, an unclassified felony, a first, second or third degree misdemeanor, an unclassified misdemeanor, or a summary offense. I also control for the total number of charges, the total number of felony charges, the total number of misdemeanor charges, and the total 'offense gravity score' of the charges (the offense gravity score is used by Philadelphia to measure the seriousness of a charge on a scale of 1-8).

[22]$Time_i$ includes dummies for each year, a cubic in the day of the year (1-365), dummies for each day of the week, and for each shift in the day (graveyard, morning, evening).

thousands of other defendants. For example, the instrument for a white female with an aggravated assault charge who had bail set by Magistrate 3 will be calculated *not just* using others with the exact same characteristics, but rather the cumulative differential effect Magistrate 3 has on the detention status of whites, females, and those facing aggravated assault charges, compared to the sample average.

The inclusion of interacted effects in the first stage increases the power of the instrument, but it also eases concerns about monotonicity violations. The data shows ample evidence that magistrates who are relatively strict among certain types of cases are relatively lenient among others. Figure 2a shows detention rates by magistrate across the entire sample. The y axis shows residuals from a regression of the pretrial detention dummy on a set of time controls; the whiskers show the 95% confidence intervals. Each bar is the average residuals per magistrate. Figures 2b-f show the same per-magistrate average detention residuals among a sample limited to those charged with each of the five most common offense types: robbery, first offense DUI, aggravated assault, drug selling and drug possession. There is clear evidence of magistrate heterogeneity in these graphs. The magistrate that is most lenient overall is the strictest on robbery charges, the magistrate who is strictest overall is the most lenient on drug selling charges, and so forth. Failing to account for this heterogeneity would mean that the instrument does not monotonically increase the likelihood of being detained for all defendants. If there is heterogeneity in treatment effects, an IV method which did not account for the heterogeneity in magistrate preferences would be biased.

The magistrate received by each defendant must be essentially random to allow for a causal interpretation of the results. Table 2 shows that pretrial detention is endogenous but that the instrument for pretrial detention is uncorrelated with observable characteristics. Each cell of the table comes from a separate regression. The dependent variables of each regression – various covariates describing the case and the defendant – are shown in the left hand side of the table. Each cell shows the coefficient on pretrial detention (Column 1) or the instrument for pretrial detention (Columns 2 and 3). Column 1 shows results for OLS regressions of each covariate on a dummy for pretrial detention, controlling only for a small set of time controls: fixed effects for each year and a cubic in the day of the year (1-365). As can be seen, pretrial detention is strongly endogenous. Those detained are facing more serious charges, have longer criminal histories, are more likely to be male, and more likely to have a graveyard-shift bail hearing. Column 2 shows results from regressing covariates on the "simple instrument", i.e. the predicted likelihood of pretrial detention based on the leave-me-out average detention rate per magistrate. Fixed effects for each year, and a cubic in the day of the year, are included to account for the fact that some magistrates work in different time periods. While pretrial detention is strongly endogenous, this simple instrument for pretrial

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detention is not. Of the 17 tests conducted, only one is statistically significant at the 5% level, no more than would be expected by chance.

Column 3 shows regressions of various covariates on the "interacted instrument" for pretrial detention, i.e. the leave-me-out predicted likelihood of detention based on the magistrate dummies interacted with three time periods, offense, criminal history, and demographics of the defendants, as described above. Once again, fixed effects for each year, and a cubic in the day of the year, are included to account for the fact that some magistrates work in different time periods. The dependent variables in Column 3 are from X_i: variables that are included as controls in the main regression but are not included as interactions with magistrate fixed effects in the first stage. These include less common crime types, general descriptors of the charges (such as the total number of felony charges), indicators for shift times or weekends, and additional measures of criminal history. Also included as a dependent variable is the "offense gravity score", which is a measure used in Philadelphia to evaluate the seriousness of the charges. Once again, the results show that the instrument for pretrial detention is exogenous to a wide variety of observable characteristics.

Appendix A shows a set of alternative randomization tests. These tests involve regressing each covariate on the magistrate fixed effects and testing for joint significance of the fixed effects. Since the randomization doesn't happen at the individual case level, but rather is a type of block randomization in which all defendants who have bail set during a given shift receive the same magistrate, I estimate the standard errors using a permutation test. The permutation test entails building 500 'false' work schedules which follow the basic parameters of the real work schedules, such as the five day rotating shifts. The number of false work schedules is limited due to the computational demands of this exercise. In all, I conduct 70 different tests using the 70 covariates available and find no evidence that magistrate assignment is anything other than random.

Figure 3 shows graphical evidence of the main results: defendants whose bail hearing is presided over by a strict magistrate are more likely to be convicted. In Figure 3a the y and x axes show residuals from a regression of conviction and pretrial detention respectively on the set of time controls described by $Time$. Figure 3b is similar except the dummies are residualized over $Cov^{sub} * T^3, X$ and $Time$. Each circle represents the average detention and conviction residuals of one of the eight magistrates. As can be seen, there is a clear positive correlation between conviction and detention which, if anything, only gets stronger once the effect of covariates have been removed.

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6   Full sample results

This section provides IV and OLS results for the full sample of defendants. The IV results are local average treatment effects: treatment effects for those defendants whose likelihood of being detained increases upon receiving a strict magistrate. While the instrument is predictive of pretrial detention across a broad spectrum of defendants, its predictive power is strongest among those at the middle range of case severity. I conduct a diagnostic test which entails dividing the sample into five groups based on their expected length of incarceration sentence (as a function of the current charge, criminal history and demographics) and regressing a dummy for pretrial detention on the instrument and controls for each subgroup. A high t-statistic on the instrument for a particular subgroup suggests that this subgroup has a high percentage of "compliers", or defendants whose likelihood of being detained increases when receiving a strict magistrate. I find that the t-statistic on the instrument is over ten for all five subgroups, but is almost twice as large (23-24) among the three middle subgroups. This suggests that there is less magistrate heterogeneity when it comes to very serious cases (murder, etc.) or low level cases (first time marijuana possession) than there is in the middle range of cases. The most common charges in this middle group include drug possession, aggravated assault, robbery, and theft. Similar to the sample averages shown in Table 1, the average number of prior arrests for this middle group is 5.47, the average age is 33, 57% are identified as black, and 83% are male.

In Table 3 I show how pretrial detention affects both conviction and the likelihood of pleading guilty using a variety of different jackknife IV specifications. The specifications vary in whether or not controls are included and in the extent to which magistrate preferences are allowed to vary over case and defendant characteristics. Column 1 shows results with no covariates and without allowing the magistrates' preferences to vary (i.e. the only exogenous variables in the first stage are the eight magistrate dummies). In Column 2 the magistrate dummies are interacted with dummies for the three time periods T^3. The standard errors decrease between the first and second column by about 10%, suggesting that allowing the magistrates to respond differently to the various changes that occur during the period of my analysis increases the power of the instrument. In the third column, detailed controls for the charges, criminal history, and demographics are introduced. The effect sizes either increase (conviction) or remain constant (guilty pleas). In Column 4 the magistrate preferences are allowed to vary according to the top five most common lead charges: drug possession, first offense DUI, robbery, selling drugs, and aggravated assault. In Column 5 I add interactions between magistrate dummies and the number of prior cases/prior violent crimes, dummies for having at least one prior case, having a detainer, and being black or female. In Column 6 I add interactions between the magistrate dummies and the other most common crime

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types: murder, burglary, theft, shoplifting, simple assault, buying drugs, marijuana possession, 2nd and 3rd degree felony firearm possession, vandalism, prostitution, and motor vehicle theft. Both effect sizes and standard errors decrease as instruments are added. This suggests that allowing the bail-setting habits of the magistrates to vary across defendant characteristics both increases the power and reduces non-monotonicity bias in the results. In particular, if treatment effects are smaller among crime types for which the monotonicity assumption is violated, then the estimates in Columns 1-3 will be biased upwards. It should be noted, however, that non-monotonicity bias will not generate spurious results if no treatment effects exist. Under the null hypothesis it would be very unlikely to see effect sizes as large as those shown in Table 3.

A t-statistic measuring the power of the first stage is shown in the bottom panel. This is the t-statistic from a regression of a dummy for pretrial detention on the instrument (the fitted values from the first stage) and the full set of controls. The t-statistic is almost three times as large in the right hand column than in the simplest specification.

Heteroskedastic-robust standard errors are shown in parentheses, and the final column also includes empirical p-values from a permutation test.[23] The permutation test entails building a number of 'false' work schedules for the magistrates. Like the real schedules, each false work schedule has a magistrate working for five days in a row on the same shift, and each magistrate only works one shift per five day period. Within these constraints, work schedules are randomly assigned to create 500 unique false work schedules. The two-stage-least-squares results are calculated for each of the false schedules and the t-statistics on the instrument for pretrial detention in the second stage are collected. The empirical p-values are the fraction of false-schedule t-statistics which are greater in absolute value than the t-statistic from the real data. This process is computationally expensive, therefore only certain specifications have this result.

My preferred specification is the one where magistrates' preferences are allowed to vary across all 17 of the most common crime types, across the criminal history, race, and gender of the defendant, and over the three time periods. The power of the instrument is greatest in this specification, the standard errors are smallest, and non-monotonicity is less likely to be a concern when magistrates preferences are allowed to vary. It should also be noted that this is the most conservative specification: the effect sizes are smaller than in the simpler specifications. I estimate that pretrial detention leads to a 6.2 percentage point increase in the likelihood of being convicted and a 4.7 percentage point increase in the likelihood of pleading guilty. Compared to the means for each dependent variable, that converts into a 13% increase in the probability of

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[23]Clustering standard errors at the magistrate or defendant level do not affect the standard errors considerably.

conviction and a 18% increase in the likelihood of pleading guilty. The empirical p-values described above are 0.016 for conviction and 0.052 for pleading guilty. These are shown in curly brackets. For reference, the parametrically estimated p-values are also shown, in double parentheses. The empirical p-values are actually slightly smaller than those estimated parametrically.

Table 4 shows how pretrial detention affects conviction rates, guilty pleas, court fees, the likelihood of being incarcerated, and both the maximum and minimum incarceration sentence.[24] Panel A shows results from the jackknife instrumental variables method with the most fully interacted specification; the first two columns are identical to the final column of Table 3. Panel B shows results from an OLS regression controlling for the full set of offense, criminal history, demographic and time controls and Panel C shows OLS results with controls only for the time and date of the bail hearing. With the exception of court fees and the incarceration dummy, results do not vary substantially between IV and OLS with controls.

The IV estimates show that pretrial detention leads to an average increase of $129 in non-bail court fees owed, counting defendants who are not convicted as receiving $0 fees. In general, defendants who are convicted in Philadelphia are required to pay court fees to cover a variety of expenses associated with the case, including court costs, victim restitution, lab tests, probation expenses, etc. Conditional on being convicted, court fees average at $611. For the tens of thousands of people convicted as a result of pretrial detention – many of whom were unable to pay even fairly small amounts of bail – these court fees may pose a significant challenge. Most defendants pay only a portion of these fees, remaining in debt to the city. 83% of defendants who were charged court fees are still in debt by December, 2015, with an average debt of $725, or 86% of the total amount. In 2011, Philadelphia hired a collection agency and began an aggressive campaign of collecting unpaid court debt dating back to 1971. This collection effort was controversial, partly because the court lacked records to back up computerized debt claims. Those who do not pay court fees face the threat of criminal prosecution, with a jail sentence of up to six months. There is no evidence, however, that criminal charges were ever filed against Philadelphia debtors (Denvir, 2012). Facing public backlash and civil rights lawsuits, Philadelphia scaled back on debt collection in 2014.

The IV results for the likelihood of being incarcerated are positive but noisy, however the results for the incarceration sentence length are more precise. Pretrial detention leads to an expected increase of 124 days in the maximum days of the incarceration sentence, a 42% increase over the mean. Detention leads to a 136 day increase in the minimum number of days before being eligible for parole. Some defendants who have been detained get released on "time-served" – in other words, they time they spent

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[24]Sentence length is coded as zero for individuals who do not receive an incarceration sentence.

detained pretrial is considered punishment for the crime. Since it was retrospectively considered punishment, I include time-served as part of the incarceration sentence. Using alternative definitions, in which time-served is not included as part of the sentence length, I estimate that pretrial detention leads to a 92 day increase in the maximum sentence and a 107 day increase in the minimum sentence. These results are noisier, although the latter result is still significant at the 10% level.

Empirical p-values for all these results are shown in curly brackets. For ease of reference, the parametrically estimated p-values are shown in double parentheses. Again, the empirical p-values are generally a little smaller than those estimated parametrically.

Table 5 provides evidence that variation in eligibility for public defense does not confound the estimates of the impacts of pretrial detention. Panel A of Table 5 is identical to Panel A of Table 4 except that there are two endogenous variables that are instrumented for with magistrate dummies: pretrial detention and a dummy for having a public defender at the time of disposition.[25] I find no statistically significant effect on having a public defender in any specification, and the coefficients on pretrial detention change only trivially. Panel B is similar to Panel A except that I add the controls for having a public defender in the second stage. Once again, the coefficients on pretrial detention change only trivially; if anything, they increase slightly in both magnitude and precision.

While not the focus of this paper, I also include estimates of the effect of pretrial detention on crime. Table 6 shows how pretrial detention impacts the likelihood of being charged with a new crime during the first six months after the bail hearing (Column 1), the likelihood of being charged with a new violent crime during the first six months after the bail hearing (Column 2), the likelihood of being charged with a new crime at least one year after the bail hearing (Column 3), the likelihood of being charged with a new crime within the second and third years after the bail hearing (Column 4), and the likelihood of being charged with a new crime within the third or fourth years after the bail hearing (Column 5). The first two columns will mostly be detecting the influence that pretrial detention has on pretrial crime. The latter three columns will mostly be detecting the impact that pretrial detention has on post-disposition crime. I define the recidivism time windows with reference to the bail hearing instead of the disposition date since the time to disposition is a function of detention status. Since those detained generally have a shorter time to disposition than those released, the effects of pretrial detention on pretrial or post-disposition crime is confounded with the length of those periods.

The sample in the latter two columns is limited to defendants for which at least

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[25]The dummy is equal to one if the defender has a public defender or a court appointed attorney; 86% of public defense is handled by a public defender. The magistrate has no say over which type of public defense is received.

three and five years (respectively) of post-bail-hearing data is available. Violent crimes consist of murder, rape, aggravated assault, robbery, and burglary. All specifications in Panel A use the fully interacted jackknife IV, all specifications in Panel B use OLS with extensive controls. Column 1 indicates that pretrial detention leads to a substantial decrease in the likelihood of a new charge. The IV estimates are about twice as large in magnitude as the OLS methods. The IV results show that detaining people for more than three days after the bail hearing (many of whom remain incarcerated for months) decreases the rate of rearrest within six months by 13 percentage points. Pretrial detention decreases the six month rate of rearrest for a violent offense by almost six percentage points. The OLS estimates on the effects of pretrial detention on violent crime are much smaller, indicating that pretrial detention predicts a decrease of less than a percentage point in six month violent crime arrest. The differences between OLS and IV in this Table is likely partly due to selection bias and partly due to the difference between the average treatment effect and the local average treatment effect.

The IV coefficients on pretrial detention in the latter three columns are negative, but only one is marginally significant at the 10% level. This is consistent with increased incapacitation due to longer incarceration sentences. The OLS results show that pretrial detention predicts an increase in post-disposition crime. This may be due to selection bias.

In Table 1B in the Appendix, I provide alternate specifications in which bail (Panel A) and non-financial release (Panel B) are the independent variables of interest. I use the jackknife IV method with the full set of magistrate interactions in the first stage. The signs of the coefficients are generally as expected: higher bail amounts are associated with higher conviction rates, court fees and incarceration, and non-financial release is associated with lower conviction rates, court fees and incarceration. The effects are less precisely estimated than the effects of pretrial detention.

Panel C of Table 1B in the Appendix shows that the main results are robust to controls for being required to make weekly or bi-weekly telephone call-ins to an automated voice message system. These telephone call-ins are the only conditions of release that the magistrates have authority to assign to defendants during the period of my analysis. A dummy for being released without monetary bail, but required to call in periodically, is instrumented for using the same magistrate interaction effects as are used to instrument for pretrial detention. This dummy will be correlated with pretrial detention, since the call-in requirement is a type of release. As such, a reduction in the coefficients on pretrial detention would not be surprising. However the coefficients on pretrial detention do not change considerably in magnitude with the addition of these controls, suggesting that the estimated impacts of pretrial detention on case outcomes is not caused by variation in the call-in requirement.

7   Subgroup effects

In Table 7 I show results for misdemeanors and felonies using both IV and OLS techniques.[26][27] The IV effect sizes of the felony sample are similar in magnitude to the full sample, but are noisy. The IV effects among misdemeanors are more precisely measured and are slightly larger than the full sample estimates, especially in relation to the means of the dependent variables. In fact, pretrial detention among misdemeanor defendants leads to a statistically significant increase in all outcomes. The effects on punishment are particularly large: those detained will be 7.6 percentage points more likely to receive a sentence of incarceration over a mean 16% incarceration rate. While the expected increase in sentence length is only a month or two, this represents more than a 100% increase relative to the mean. The large incarceration effects among misdemeanor defendants may be partly explained by defendants who receive a sentence of "time-served", which is more common among misdemeanors. Using alternative definitions of sentence length in which time spent detained pretrial is subtracted from the incarceration sentence, pretrial detention is estimated to lead to a 38 day increase in the maximum days and an 11 day increase in the minimum days.

Figure 4 shows how the impacts on conviction vary across offense. The top panel shows OLS results for each subgroup of defendants facing the charge shown on the left. The dot is the coefficient and the line represents the 95% confidence interval. The bottom panel is similar to the top, except the jackknife IV method (using only the 8 magistrate dummies, no interactions) is used. The IV results are included for completeness, however the wide confidence intervals preclude meaningful inference. The subgroup IV results with the widest intervals were omitted for visual clarity. The OLS results are much more precisely estimated and the effects vary widely by offense type. While OLS results may exhibit bias, it is unlikely that the bias for each subgroup would exactly cancel out the heterogeneity in effect size.

Several explanations are consistent with the variation in effect sizes shown in the OLS regressions. Generally, effect sizes appear larger among more serious crimes, but there are exceptions. Selling drugs is considered a serious crime in Philadelphia, as is illegal firearm possession. The effect sizes are relatively small for these offenses. Conversely, simple assault is a less serious crime, but the effect size is large for this category. Another potential explanation has to do with the strength of the evidence that tends to be available in each type of case. Evidence can be difficult to refute in certain types of crimes, generally those in which the defendant is caught in the illegal

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[26]The felony sample is defined as those who were charged with at least one felony at the time of the bail hearing; many of these had their charges downgraded to misdemeanors only by the time of the arraignment.

[27]The IV specifications allow the magistrate preferences to vary across time and across defendant characteristics, as shown in Column 6 of Table 3.

act: drug possession/sale, DUI, illegal firearm possession. In other types of crimes, the inculpatory evidence is weaker. It is possible that there is less room for extra-legal factors such as detention status to influence case outcomes when the evidence is strong than in cases where the evidence is more contestable. When the prosecution's evidence is weak the defendant's success depends more on her ability to gather exculpatory evidence, confer with her lawyer, or engage in strategic delay tactics. These are all more difficult for those in jail. Further, pretrial detention may change the defendants' reference point so that incarceration is the default and freedom seen as a gain. Since weaker evidence cases will have higher variance in the outcome, risk aversion over gains may tilt the defendant towards pleading guilty.

I develop two measures of the strength of the evidence that tends to be available in different types of cases. The first measure is a survey of Philadelphia's criminal justice professionals, in which I ask them to rate the strength of evidence that is typical of different offense types. The second measure is the average conviction rate per offense type, with the assumption that offense types where the evidence tends to be strongest will have the highest rates of conviction. While the two methods vary somewhat in the ranking, the results are broadly similar. The offenses shown in Figure 4 are ordered by the average of the two rankings, with the strong-evidence cases on the top and the weak-evidence cases on the bottom. Indeed, effect sizes do appear generally larger among weaker-evidence offense types. While suggestive, more research is necessary to understand why pretrial detention affects case outcomes more in certain types of cases than others.

Tables 1C, 1D, and 1E in the Appendix show IV results for blacks, whites, young defendants, older defendants, those with one or no prior arrests, and those with more extensive criminal history. There is suggestive evidence that effect sizes are slightly large for those with limited prior history, particularly with reference to the means of the dependent variables, but the confidence intervals are too large to draw definitive conclusions. The effect sizes do not appear to differ substantially across the age or race of the defendant.

While the IV results are generally too noisy to demonstrate conclusive evidence of treatment effect heterogeneity, this is a far cry from showing definitive evidence that treatment effects are homogenous. When the monotonicity assumption is violated, small differences in treatment effects across groups can result in large biases. Under clear violations of monotonicity, the burden of proof is on the researcher to demonstrate that treatment effects are homogenous. A conservative – and common sense – approach is to assume that there is at least some treatment effect heterogeneity, even if the IV method can't provide conclusive evidence about where it lies.

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8   Socio-economic disparities

The results shown in the previous two sections demonstrate that pretrial detention has serious consequences beyond simply the loss of freedom during the pretrial period. This is particularly concerning if there are socio-economic disparities in detention rates. I find that black defendants are about 40% more likely to be detained than non-black defendants, and defendants from low-income neighborhoods are about 17% more likely to be detained than those from wealthier neighborhoods. About half of this gap is due to the fact that, on average, black defendants and those from poor neighborhoods are facing more serious charges and have lengthier criminal records, and thus have higher bail. Conditional on the charge and criminal record, bail is very similar across race and neighborhood income level. About half of the gap in detention rates across race and neighborhood income is due to differences in the likelihood of posting a given amount of bail. For example, about 46% of black defendants with bail set at $5000 post bail, compared to 56% of non-black defendants.

In Panel A of Table 8 I test to see if there are differences in bail amounts across race and neighborhood income, conditional on the charge, criminal record, and age of the defendants. The dependent variables in each column are dummies for receiving non-monetary bail, low bail (bail greater than zero and less than or equal to $2500), medium bail (bail greater than $2500 and less than or equal to $10,000) and high bail (bail greater than $10,000). The independent variables are a dummy for being labeled black in the data and a dummy for living in the lowest quintile of average zip code income, as measured by IRS tax returns. Each regression controls for a detailed set of variables describing the current charges, the criminal history, the time and date of the bail hearing, and age. The sample is limited to those for whom zip code data is available.

While some of the results are statistically significant, they tend to be small in magnitude. Furthermore, the results vary in sign in a manner that belies a clear trend. Bail is neither systematically higher nor systematically lower for black defendants or those from low-income neighborhoods, conditional on charge, criminal history and age. These results cannot be interpreted "causally" because there may be differences across the groups that are not captured in the control variables. Furthermore, within a Rubin Causal Model framework, the causal impact of characteristics that are deeply intertwined with many facets of life, such as race, are hard to conceptualize. However as a descriptive statement, bail levels differ only trivially across the race and neighborhood wealth of defendants, conditional on the charge, criminal record, and age.

There have recently been a number of civil rights lawsuits which contend that bail that doesn't take ability to pay into account is unconstitutional.[28] The Department of

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[28]Varden v. City of Clanton, Walker v. Calhoun, O'Donnell V. Harris County, Buffin/Patterson v. City

Justice filed an amicus brief in support of one of these suits, stating "...bail practices that incarcerate indigent individuals before trial solely because of their inability to pay for their release violates the Fourteenth Amendment."[29] If magistrates were taking ability to pay into account as they set bail we would expect bail to be lower for low-income defendants. To the extent that race and neighborhood income levels are a proxy for ability to pay, I find no evidence that magistrates are setting bail lower for defendants who are less able to afford it.

Panel B of Table 8 shows the fraction of defendants within each bail category who post bail. For example, Row 1 of Column 2 shows the fraction of black defendants with low bail who post bail. Row 2 of Column 2 show the fraction of defendants whose race is labeled white or unknown with low bail who post bail.[30] Across all bail categories, black defendants are less likely to post bail than non-black defendants, and defendants from low-income neighborhoods are less likely to post bail than defendants from high-income neighborhoods.

Panel A of Table 9 compares the actual detention rate of black and non-black defendants to the detention rate each group is predicted to have if they posted bail at the same rate as the other group. The calculations for these predictions are shown in Equations 3 and 4. Equation 3 shows the predicted detention rate for defendant j who is in group J, based on the average detention rate for defendants with the same amount of bail as defendant j but who are in group S. For example, if J is the group of black defendants and S is the group of non-black defendants, $Predicted\_Detention_j^S$ for a black defendant with bail set at \$5000 would be the average detention rate of non-black defendants who have bail set at \$5000. The predicted detention rate for entire group J, based on detention rates of group S ($Predicted\_Detention^{J,S}$) is thus the average predicted detention rate of all defendants in group J. Defendants who have some sort of detainer placed on them that would prevent them from being released even if they posted bail are removed from the sample for the purposes of this exercise.[31]

$$Predicted\_Detention_j^S = \frac{\sum_s^S \mathbb{I}(bail_s = bail_j) * detention_s}{\sum_s^S \mathbb{I}(bail_s = bail_j)} \qquad (3)$$

$$Predicted\_Detention^{J,S} = \frac{1}{J} \sum_j^J Predicted\_Detention_j^S \qquad (4)$$

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and County of San Francisco, etc.

[29]Walker v. Calhoun, 2016

[30]Column 1 shows that 1-2% of those with non-monetary bail do not post bail. The bail information is the original bail amount, some defendants have the bail revised upwards if the prosecution appeals the decision of the magistrate.

[31]A very tiny minority of defendants – less than 200 for all groups – have bail set at a level where there are no defendants from the other group. These have been dropped.

Row 1 of Panel A of Table 9 shows the actual detention rates for both black defendants and non-black defendants. Black defendants are 12 percentage points more likely to be detained than defendants labeled white or unknown. Part of this race gap is due to the fact that black defendants, on average, are facing more serious charges and have higher levels of bail. Row 2 isolates the differences in detention rates that are due solely to differences in the likelihood of posting bail. Column 1 of Row 2 shows the predicted rates of detention for black defendants if they posted bail at the same rate as non-black defendants. This is $Predicted\_Detention^{J,S}$, where group J consists of black defendants and group S consists of non-black defendants. While the actual rate of detention for black defendants is .427, the predicted rate is only .369. Black defendants are 5.8 percentage points more likely to be detained than they would be if they had posted bail at the same rate as non-black defendants. Similarly, non-black defendants are 5.2 percentage points *less* likely to be detained than if they posted bail at the same rate as black defendants. Thus, about half of the total race gap in detention rates is due to the fact that black defendants are less likely to post bail.[32]

Panel B of Table 9 conducts a similar exercise, comparing actual versus predicted rates of detention for defendants from the lowest quintile and highest quintile of the average zip code income distribution. The detention rate for defendants from high-income zip-codes is .402; the detention rates for defendants from low-income zip-codes is .341. In relative terms, defendants from low-income zip-codes are 17% more likely to be detained than those from wealthier neighborhoods. As above, approximately half of the total gap in detention rates across neighborhood income is due to the fact that defendants from low-income neighborhoods are less likely to post bail.

Both race and the average neighborhood income are expected to correlate with the financial means of the defendant. In fact, the average household income for blacks in Philadelphia is 2/3 that of white households (Ingram, 2007). The most straightforward explanation for the disparities in the likelihood of posting bail across race and neighborhood income is that, on average, black defendants and those from low-income neighborhoods are less able to afford the bail that has been set.

9   Conclusion

Right now there is a wave of momentum in bail reform that dwarfs any seen in decades. In the last several years, New Jersey, Kentucky, Colorado, Maryland, New Mexico, Chicago, New York City and many other places have committed to or imple-

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[32]If this exercise is done in regression format, where a dummy for pretrial detention is regressed on a fully saturated set of dummies for the bail amount and an indicator for being black, the coefficient on "black" has a t-statistic of 40.

mented pretrial reform. Twenty-six cities are implementing new pretrial risk assessment regimes in partnership with the Laura and John Arnold Foundation and 20 cities are developing pretrial reform proposals with a $75 million fund from the MacArthur Foundation. Yet despite all this activity, research on the pretrial period is limited.

Using a natural experiment in Philadelphia where the likelihood of being detained pretrial is exogenously affected by the magistrate who presides over the bail hearing, I find that pretrial detention leads to a 13% increase in the likelihood of being convicted, mostly by increasing the likelihood that defendants, who otherwise would have been acquitted or had their charges dropped, plead guilty. Pretrial detainees will owe more in court fees and receive longer incarceration sentences than similarly situated releasees.

I also find that black defendants are about 40% more likely to be detained than non-blacks, and defendants from low-income neighborhoods are 17% more likely to be detained than those from wealthier neighborhoods. While part of these gaps are explained by differences in the charges and criminal record – and therefore differences in the bail amount – another part is explained by differences in the likelihood of posting bail. If black defendants and those from low-income neighborhoods posted bail at the same rate as non-blacks or those from wealthier neighborhoods, about half of the gap in detention rates would disappear. It is likely that at least part of the disparities in the likelihood of posting bail are due to disparities in wealth, income, and the access to credit, suggesting that the use of monetary bail leads to socio-economic disparities in pretrial detention. Since pretrial detention affects case outcomes, monetary bail would then lead to socio-economic disparities in conviction and sentencing.

The downstream consequences of pretrial detention suggest that the bail hearing should indeed be thought of as a 'critical-stage', and, since defendants have a constitutional right to counsel at all 'critical stages' of the criminal proceedings, defense attorneys should be provided to the indigent. However, as of 2008, only ten states were uniformly providing counsel at the first appearance (Colbert, 2011).[33] More generally, the impact that pretrial detention has on the outcome of a case underlines the importance of ensuring that only defendants who pose a risk to society or to the integrity of the criminal proceedings should be detained. Bail hearings that last a minute long, occur over videoconference, and have no lawyers present are unlikely to be effective in determining which defendants can be safely released. Taking more time to evaluate evidence, hearing arguments from counsel, and providing increased training to magistrates is likely to increase the efficacy of the decision making process and reduce the likelihood that low risk defendants are incarcerated before they are convicted. Finally, the results of this paper speak to the importance of eliminating socio-economic dispar-

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[33]Philadelphia is currently implementing reforms to provide a public defender to confer with the client before the bail hearing, and to present any mitigating evidence to the magistrate.

ities in pretrial detention. The fact that black defendants and those from low-income neighborhoods are more likely to be detained solely because they are less likely to post bail suggests that a reduced reliance on monetary bail will reduce disparities in detention and thus disparities in conviction rates and incarceration.

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Table 1: Summary statistics

	Released	Detained	Total
Age	32.8	32.0	32.5
Male	0.79	0.88	0.83
White	0.30	0.26	0.28
Black	0.52	0.65	0.57
Unknown/missing race	0.15	0.06	0.11
Charged with selling drugs	0.12	0.13	0.12
Charged with robbery	0.02	0.14	0.07
Charged with drug possession	0.18	0.06	0.13
Charged with aggravated assault	0.07	0.11	0.09
Charged with 1st offense DUI	0.10	0.02	0.06
Number of prior cases	3.90	6.28	4.88
Has felony charge at time of bail hearing	0.36	0.72	0.51
Case proceeds to felony court	0.19	0.40	0.28
Number of charges per case	4.98	6.56	5.63
Bail	$3,413	$61,974	$26,844
Non-financial release	0.54	0.01	0.33
Detained>3 days	0	1	0.41
Detained at time of disposition	0	0.60	0.25
All charges dropped or dismissed	0.48	0.48	0.48
Case went to trial	0.32	0.19	0.27
Not guilty on all charges	0.03	0.03	0.03
Guilty of at least one charge	0.49	0.49	0.49
Pled guilty to at least one charge	0.21	0.33	0.26
Court fees charged	$387	$206	$312
Sentenced to incarceration	0.18	0.32	0.24
Maximum days of incarceration sentence	94	576	292
Minimum days of incarceration before parole eligibility	39	322	155
Observations	195,340	136,631	331,971
Conditional summary statistics			
Court fees charged (cond. on conviction)	$409	$753	$611
Sentenced to incarceration (cond. on conviction)	0.46	0.67	0.49
Max. days of incarc. sentence (cond. on incarceration)	529	1736	1213
Min. days before parole eligibility (cond. on incarceration)	220	971	645

Note: "Released" is defined as released from pretrial custody within three days after the bail hearing, and "Detained" is defined as detained pretrial for at least four days. The statistic shown is the mean and, unless otherwise indicated, variables are dummies where 1 indicates the presence of a characteristic. Age is measured in years, those marked "Number..." are count variables, and those expressed in dollar amounts are currency. The sentence is coded as zero if the defendant did not receive an incarceration sentence. The summary statistics in the bottom panel are limited to those who are convicted (top two rows) or receive an incarceration sentence (bottom two rows).

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Table 2: Randomization test

	(1) OLS	(2) Simple instrument	(3) Interacted instrument
White	-0.0399****	0.0834	
	(0.00158)	(0.0631)	
Male	0.0905****	-0.00484	
	(0.00126)	-0.00484	
At least one prior charge	0.140****	-0.0485	
	(0.00143)	(0.0600)	
Robbery	0.127****	0.00994	
	(0.00101)	(0.0364)	
First time DUI	-0.0833****	-0.0429	
	(0.000760)	(0.0335)	
Selling drugs	0.00634****	0.0170	
	(0.00117)	(0.0466)	
Aggravated assault	0.0444****	-0.00302	
	(0.00105)	(0.0395)	
Age	-0.901****	-1.700	0.377
	(0.0398)	(1.574)	(0.602)
Prior felony arrests	0.819****	0.559**	-0.0623
	(0.00772)	(0.274)	(0.108)
Prior convictions	0.779****	-0.127	-0.0796
	(0.00902)	(0.337)	(0.128)
Offense gravity score	9.107****	-0.675	0.158
	(0.0422)	(1.673)	(0.365)
Number felony charges	3.193****	-0.494	-0.0167
	(0.0168)	(0.673)	(0.184)
Rape	0.0156****	-0.0104	0.00116
	(0.000372)	(0.0128)	(0.00457)
Resisting arrest	0.0108****	-0.0273	-0.00407
	(0.000591)	(0.0225)	(0.00878)
Disorderly conduct	-0.00712****	0.00861	0.00254
	(0.000420)	(0.0171)	(0.00274)
Graveyard shift	0.0311****	0.0753	0.00799
	(0.00165)	(0.0650)	(0.0284)
Weekend shift	-0.000252	0.0262	0.0197*
	(0.000635)	(0.0252)	(0.0113)
Observations	331971	331971	331971

Heteroskedastic-robust standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: The dependent variables are shown on the left hand side. In Column 1 the independent variable is a dummy for pretrial detention, in Column 2 it's the "simple instrument" for pretrial detention (the predicted likelihood of detention based on the magistrate dummies) and in Column 3 it's the "interacted instrument" (the predicted likelihood of detention based on the magistrate dummies interacted with three time periods, offense, criminal history and demographics.) Each regression controls for the year and season of the bail hearing to account for the fact that some magistrates work in different time periods.   32

Table 3: How does pretrial detention affect conviction rates and guilty pleas?

Panel A: Full sample (IV)	Conviction (mean dep. var.= 0.49)					
	(1)	(2)	(3)	(4)	(5)	(6)
Pretrial detention	0.167**	0.180***	0.282***	0.119***	0.0907**	0.0620**
	(0.0736)	(0.0655)	(0.0868)	(0.0412)	(0.0364)	(0.0291)
						{0.016}
						((0.032))
Panel B: Full sample (IV)	**Guilty pleas (mean dep. var.=0.25)**					
	(1)	(2)	(3)	(4)	(5)	(6)
Pretrial detention	0.124**	0.174***	0.177**	0.102***	0.0536*	0.0469*
	(0.0619)	(0.0563)	(0.0776)	(0.0366)	(0.0324)	(0.0262)
						{0.052}
						((0.073))
Magistrate X 3 time periods		Y	Y	Y	Y	Y
Magistrate X top 5 crimes				Y	Y	
Magistrate X crim. history					Y	Y
Magistrate X demographics					Y	Y
Magistrate X top 17 crimes						Y
Time controls	Y	Y	Y	Y	Y	Y
Covariates			Y	Y	Y	Y
Observations	331971	331971	331971	331971	331971	331971
First stage t-stat.	15.56	17.59	20.43	33.25	36.75	44.64
Mean indep. var	0.41	0.41	0.41	0.41	0.41	0.41

Heteroskedastic robust standard errors in parentheses

Empirical p-values in curly brackets

Non-parametric p-values in double parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: The dependent variable in Panels A and B respectively are dummies for being convicted on at least one charge and pleading guilty to at least one charge. The exogenous variables in the first column are the eight magistrate dummies; in the subsequent columns they include interactions between the magistrate dummies and three time period fixed effects, the five most common crime types, a variety of criminal history variables, defendant demographics, and the remainder of the 17 most common crime types. The first two columns control only for the time and date of the bail hearing, all subsequent columns include the full set of controls for offense, criminal history and demographics as described in Section 5. Empirical p-values as derived from a permutation test are shown in curly brackets and parametrically estimated p-values are shown in double parentheses. The t-statistic on the first stage of the jackknife IV are shown in the sub-panel, as are the means of the independent variables. A linear jackknife instrumental variables regression is used. The R^2 is not reported due to difficulties of interpreting this statistic in an IV regression.

Table 4: Full sample results - jackknife IV and OLS

	(1) Conv- iction	(2) Guilty plea	(3) Court Fees	(4) Incarc- eration	(5) Max days	(6) Min days
Panel A: Full sample (IV)						
Pretrial detention	0.0620**	0.0469*	129.5****	0.0186	124.7*	136.4**
	(0.0291)	(0.0262)	(33.26)	(0.0249)	(74.40)	(62.61)
	{0.016 }	{0.052}	{0.000}	{0.466}	{0.054}	{0.008}
	((0.032))	((0.073))	((0.000))	((0.458))	((0.095))	((0.030))
Panel B: Full sample (OLS with full controls)						
Pretrial detention	0.0333****	0.0566****	-103.5****	0.0976****	133.7****	67.78****
	(0.00197)	(0.00181)	(2.618)	(0.00166)	(3.463)	(2.539)
Panel C: Full sample (OLS only time controls)						
Pretrial detention	0.000163	0.106****	-180.7****	0.154****	480.0****	281.8****
	(0.00176)	(0.00156)	(2.078)	(0.00154)	(5.766)	(4.843)
Observations	331971	331971	331971	331971	331971	331971
First stage t	44.64	44.64	44.64	44.64	44.64	44.64
Mean dep. var.	0.49	0.26	312	0.24	292	155

Heteroskedastic robust standard errors in parentheses

Empirical p-values in curly brackets

Non-parametric p-values in double parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: This table shows how pretrial detention affects various case outcomes using both a jackknife IV regression (Panel A), an OLS regression with a full set of controls (Panel B), and an OLS regression with only time controls. The outcome variables are dummies for being convicted/pleading guilty, total non-bail court fees in dollars, a dummy for whether or not the defendant receives an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. In all of the IV specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history and demographics of defendants. The t-statistic on the first stage of the jackknife IV are shown in the sub-panel, as are the means of the dependent variables. All regressions include the full set of controls as described in Section 5.

Table 5: Robustness checks

Panel A: Instrumenting for public defender (Full sample, IV)						
	(1) Conv-iction	(2) Guilty plea	(3) Court Fees	(4) Incarc-eration	(5) Max days	(6) Min days
Pretrial detention	0.0625**	0.0470*	120.5****	0.0230	147.5*	149.0**
	(0.0304)	(0.0271)	(33.17)	(0.0255)	(79.02)	(66.97)
Public defender	0.00339	0.00115	-67.48	0.0329	169.6	93.54
	(0.0539)	(0.0481)	(72.23)	(0.0477)	(197.2)	(170.7)
Panel B: Controlling for public defender (Full sample, IV)						
	(1) Conv-iction	(2) Guilty plea	(3) Court Fees	(4) Incarc-eration	(5) Max days	(6) Min days
Pretrial detention	0.0688**	0.0520**	126.0****	0.0246	119.9	131.9**
	(0.0285)	(0.0257)	(33.18)	(0.0246)	(73.50)	(61.78)
Public defender	0.0394****	0.0292****	-36.43****	0.0421****	11.65	-4.382
	(0.00366)	(0.00330)	(4.531)	(0.00314)	(10.03)	(8.544)
Observations	331971	331971	331971	331971	331971	331971
Mean dep. var.	0.49	0.26	312	0.24	292	155
First stage t on detention	44.64	44.64	44.64	44.64	44.64	44.64
First stage t on pub. def.	31.08	31.08	31.08	31.08	31.08	31.08

Standard errors in parentheses

Heteroskedastic-Robust Standard Errors

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: This table presents robustness checks for the main results. Panel A instruments for two endogenous variables: a dummy for having a public defender and the pretrial detention dummy. Panel B includes adds the controls for having a public defender into the second stage. The outcome variables are dummies for being convicted/pleading guilty, total non-bail court fees in dollars, receiving an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. In all specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history and demographics of defendants. The t-statistics on the instrument for pretrial detention, the t-statistic on the instrument for public defense, and the means of the dependent variables are shown in the subpanel. All regressions include the full set of controls as described in Section 5.

Table 6: Impacts on crime

	(1) Charge within 6 mos.	(2) Violent charge within 6 mos.	(3) Charge after first year	(4) Charge in 2nd-3rd yr	(5) Charge in 4th-5th yr
Panel A: Full sample (IV)					
Pretrial detention	-0.129****	-0.0581****	-0.0440	-0.0531*	-0.0290
	(0.0256)	(0.0146)	(0.0286)	(0.0316)	(0.0355)
Panel B: Full sample (OLS with full controls)					
Pretrial detention	-0.0684****	-0.00547****	0.0109****	0.000959	0.0206****
	(0.00166)	(0.000939)	(0.00194)	(0.00212)	(0.00251)
Observations	331971	331971	331971	279941	182460
First stage t	44.64	44.64	44.64	42.12	38.36
Mean dep. var.	0.195	0.049	.507	0.361	0.293

Standard errors in parentheses

Heteroskedastic-Robust Standard Errors

$*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$, $****$ $p < 0.001$

Note: This table shows how pretrial detention affects future crime using both a jackknife IV regression (Panel A) and an OLS regression with a full set of controls (Panel B). The outcome variables are dummies for being charged with a new crime during the six months after the bail hearing, charged with a violent crime during the six months after the bail hearing, charged with a new crime at any point after the first year after the bail hearing, charged with a new crime within the second or third year after the bail hearing, and charged with a new crime in the fourth or fifth year after the bail hearing. In all of the IV specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history and demographics of defendants. The t-statistic on the first stage of the jackknife IV is shown in the sub-panel, as are the means of the dependent variables. All regressions include the full set of controls as described in Section 5.

Table 7: Comparing results for misdemeanors and felonies

	(1) Conviction	(2) Guilty plea	(3) Court Fees	(4) Incarceration	(5) Max days	(6) Min days
Panel A: Misdemeanors (IV)						
Pretrial detention	0.0766**	0.0577*	77.55**	0.0759***	55.82**	26.62**
	(0.0363)	(0.0295)	(38.03)	(0.0281)	(21.95)	(12.09)
Panel B: Misdemeanors (OLS with controls)						
Pretrial detention	0.0148****	0.0523****	-15.71****	0.0490****	38.42****	19.25****
	(0.00298)	(0.00249)	(3.083)	(0.00213)	(2.110)	(1.401)
Observations	163236	163236	163236	163236	163236	163236
First stage t	37.91	37.91	37.91	37.91	37.91	37.91
Mean dep. var.	0.50	0.16	$351	0.16	48	18
Mean indep. var.	0.23	0.23	0.23	0.23	0.23	0.23
Panel C: Felonies (IV)						
Pretrial detention	0.0513	0.0391	139.3***	-0.0257	182.3	207.0*
	(0.0434)	(0.0414)	(53.69)	(0.0398)	(139.9)	(119.3)
Panel D: Felonies (OLS with controls)						
Pretrial detention	0.0512****	0.0589****	-172.4****	0.131****	188.2****	93.83****
	(0.00267)	(0.00260)	(4.016)	(0.00245)	(5.662)	(4.120)
Observations	168735	168735	168735	168735	168735	168735
First stage t	31.91	31.91	31.91	31.91	31.91	31.91
Mean dep. var.	0.47	0.35	$274	0.32	528	288
Mean indep. var.	0.58	0.58	0.58	0.58	0.58	0.58

Standard errors in parentheses

Heteroskedastic-Robust Standard Errors

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: This table shows effect sizes in misdemeanor crimes (Panels A and B) and felonies (Panel C and D). The outcome variables are dummies for being convicted/pleading guilty, total non-bail court fees in dollars, receiving an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. In all IV specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history and demographics of defendants. The t-statistic on the first stage of the jackknife IV is shown in the sub-panel, as are the means of the dependent and independent variables. All regressions include the full set of controls as described in Section 5.

Table 8: Bail differences by race and neighborhood income

Panel A				
	(1)	(2)	(5)	(6)
	Non-monetary	Low bail	Med bail	High bail
Low-income zip	-0.00104	-0.000258	0.00553***	-0.00423***
	(0.00158)	(0.00164)	(0.00178)	(0.00141)
Black	-0.00957****	0.00275*	-0.00172	0.00854****
	(0.00140)	(0.00146)	(0.00158)	(0.00125)
Observations	295008	295008	295008	295008
Mean dep var	.33	.19	.27	.21
R^2	0.438	0.121	0.202	0.412

Panel B				
	(1)	(2)	(5)	(6)
	Non-monetary	Low bail	Med bail	High bail
Fraction posting, black	.98	.62	.47	.12
Fraction posting, white/unknown	.99	.69	.56	.22
Fraction posting, low-income zip	.98	.61	.49	.13
Fraction posting, high-income zip	.98	.67	.53	.18

Standard errors in parentheses

Heteroskedastic-Robust Standard Errors

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: The dependent variables in Panel A, from left to right, are a dummy for receiving non-monetary bail, bail greater than zero and less than or equal to \$2500, bail greater than \$2500 and less than or equal to \$10,000, and bail greater than \$10,000. All regressions include extensive controls describing the offense, the criminal history, and the age of the defendant, as well as controls for the time and date of the bail hearing. Panel B shows the fraction of defendants who post bail in various subsamples; the subsamples are defined by the columns and rows. For example, Row 1 of Column 2 shows the fraction of black defendants with bail set in the "low bail" category (bail greater than zero and less than or equal to \$2500) who post bail within three days of the bail hearing.

Table 9: Socio-economic disparities in pretrial detention rates

| Panel A		
	(1)	(2)
	Black	White/unknown
Actual detention rates	.427	.305
Predicted detention rates, based on white/unknown	.369	.305
Predicted detention rates, based on blacks	.427	.357
Panel B		
	(1)	(2)
	Low-income zip	High-income zip
Actual detention rates	.402	.341
Predicted detention rates, based on high-income zip	.371	.341
Predicted detention rates, based on low-income zip	.401	.370

Note: The second and third rows of the top panel show the predicted rates of detention, where the predictions are built off of the non-black and black sample respectively. For example, the first column of the second row shows the predicted rate of detention for black defendants, if black defendants posted bail at the same rate as non-black defendants. The second and third rows of the bottom panel show the predicted rates of detention, where the predictions are built off of defendants from the highest quintile and lowest quintile average income zip codes respectively. Defendants who have some sort of detainer that would prevent them from posting bail are removed from the sample.

Figure 1: Descriptive graphs

(a) Magistrate leniency and public defense
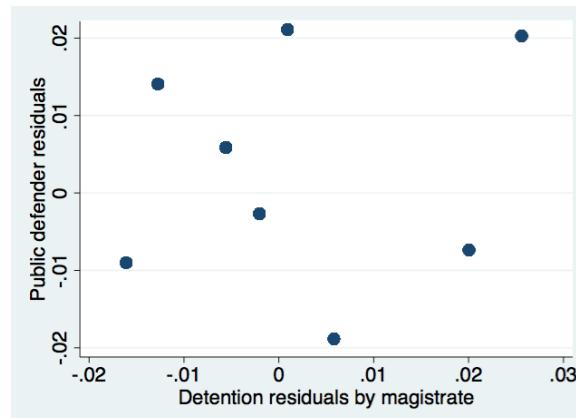
(b) Days detained pretrial, conditional on being detained more than three days
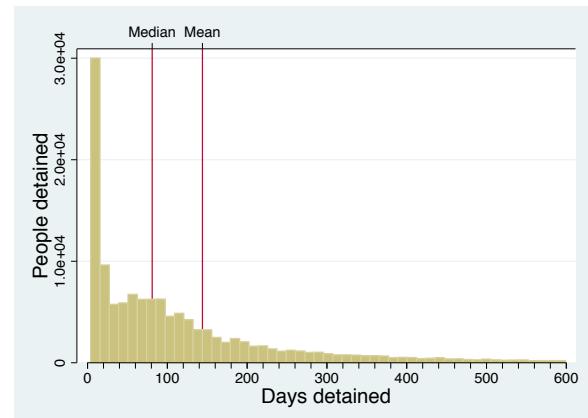
(c) Distribution of bail
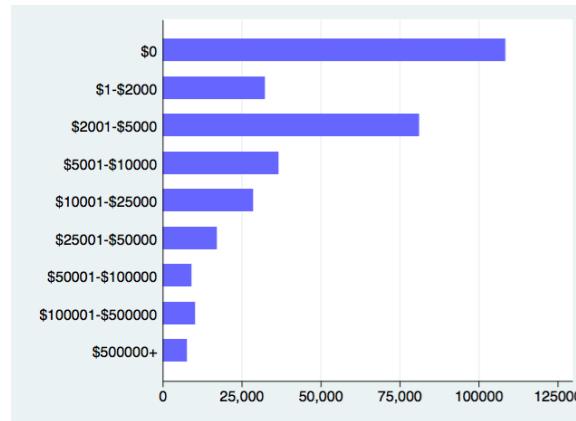
(d) Bail amounts and detention status
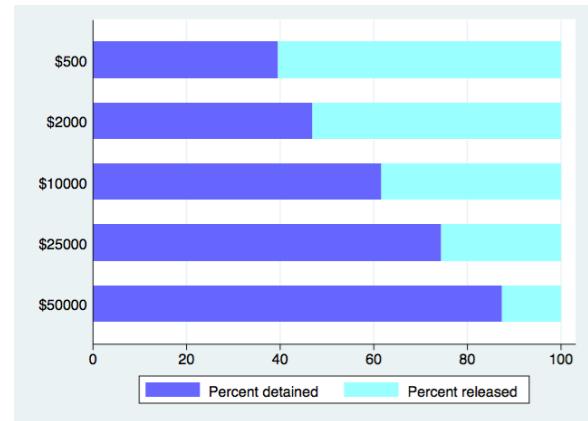
Note: Figure 1a shows the relationship between pretrial detention and having a public defender. Each dot represents the average per magistrate. Both pretrial detention and public defense have been residualized against time controls to account for the fact that some magistrates work in different time periods. Figure 1b shows the average number of days detained for those who are detained for more than three days after the bail hearing. Figure 1c shows the distribution of bail amounts. The x axis shows the number of defendants who have bail set within each interval. Figure 1d shows the percentage released and detained at a variety of bail levels among defendants who did not have a detainer placed on them (i.e. were free to leave if they posted bail).

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Figure 2: Average detention rates by magistrate for different offense types

(a) All cases

(b) Robbery

(c) DUI, 1st offense

(d) Aggravated assault


(e) Drug selling

(f) Drug possession
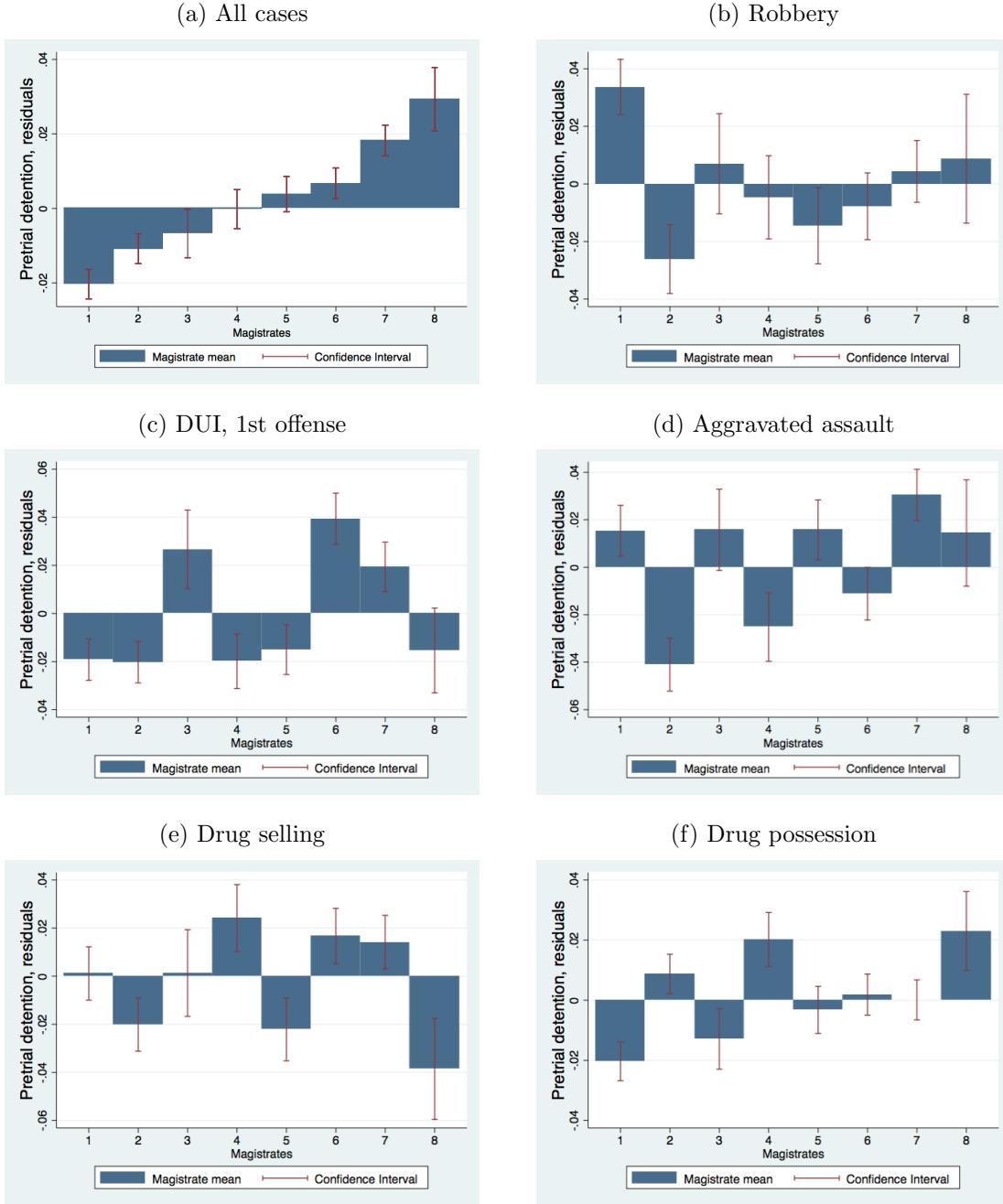
Note: This figure shows pretrial detention rates by magistrate over the whole sample (Figure 2a), and for the five most common offense categories (Figures 2b-f). The numbers 1 through 8 delineate the different magistrates. The y axes show the residuals from a regression of pretrial detention on time controls; each bar represents the per-magistrate average of the residuals. The error bars indicate the 95% confidence intervals for the mean. The numbering of the magistrates is consistent across all samples. The bar heights in Figures 2b-f are not expected to sum to the bar heights in Figure 2a, as not all offense categories are shown.

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Figure 3: Visual IV

(a) Full sample – conviction rates and pretrial detention are residualized over time controls
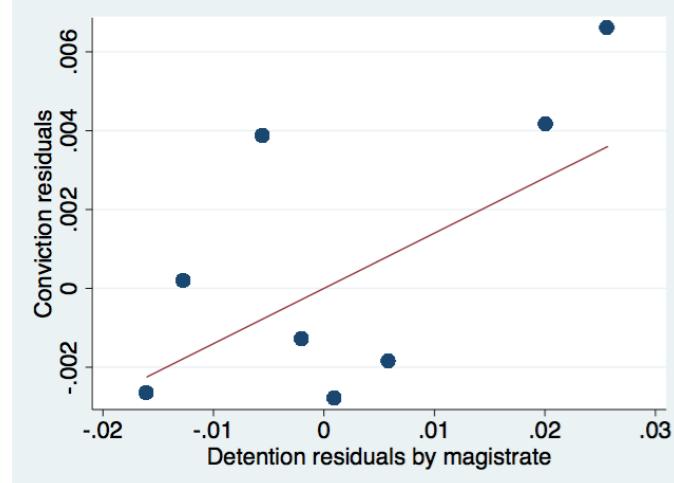

(b) Full sample – conviction rates and pretrial detention are residualized over time controls, offense, criminal history and demographics
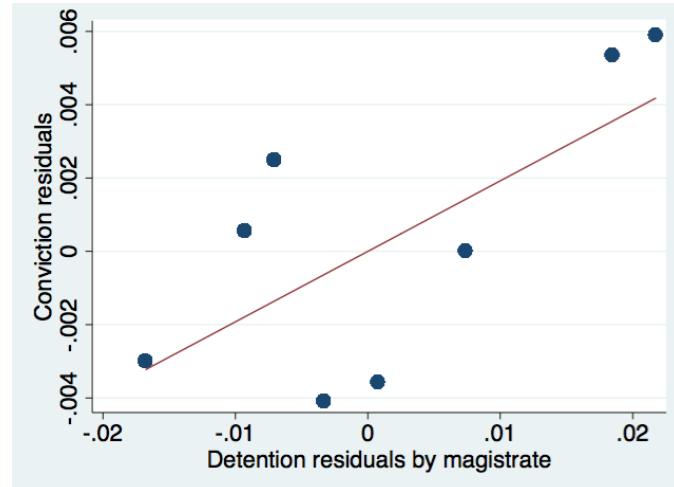
Note: The y and x axes in Figure 3a show the residuals from a regression of a dummy for conviction and pretrial detention (respectively) on controls for the time and date of the bail hearing. Figure 3b is the same, except conviction and detention have been residualized over offense, criminal history and demographic covariates as well as time controls. The circles in Figures 3a-b show the average detention and conviction residuals for each magistrate.

Figure 4: Effect sizes by offense

(a) OLS
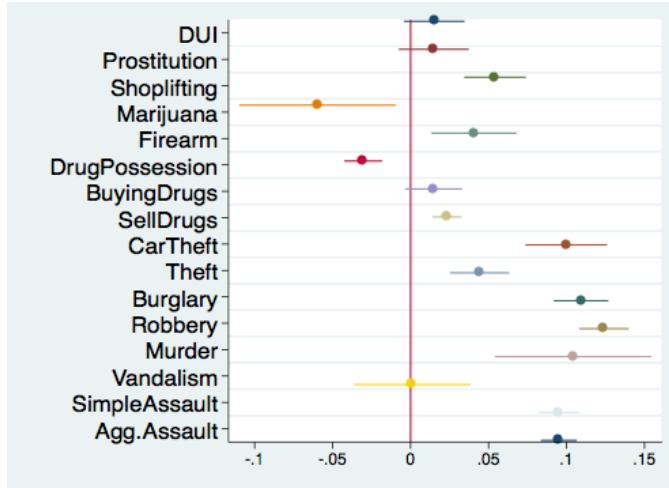
(b) IV
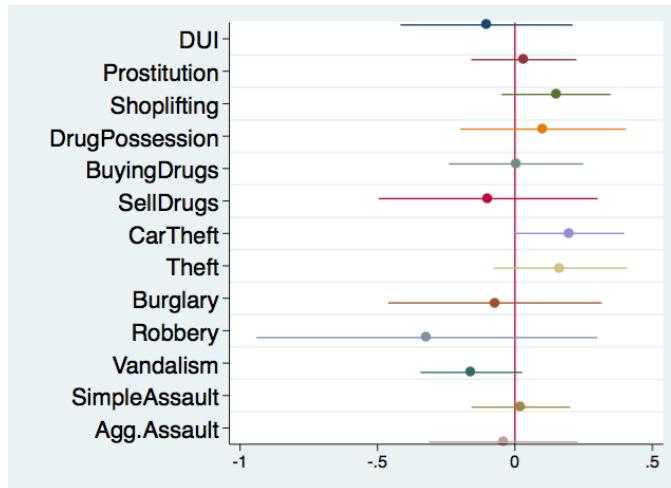
Note: The above coefficient plots show the OLS and IV estimates of the impact pretrial detention has on conviction rates for different offenses, as labeled on the left. Each dot represents the estimated coefficient on pretrial detention, the line represents the 95% confidence interval. Murder, 2nd and 3rd degree illegal firearm charges, and possession of marijuana are left off of the IV plot since their wide confidence intervals make the other estimates hard to see.

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Appendix

A   Randomization test

For each of 70 covariates describing the offense, criminal history, and demographics of the defendant I run the following regression:

$$Cov_i = \alpha + Magistrate_i * \beta + Time_i * \psi + e_i$$

I collect and store the F statistic from a test of joint significance on β. I then build 500 'false' work schedules for the magistrates.[34] The false schedules follow parameters similar to the real schedules: each magistrate works five days on each shift, there are three shifts per day, and each magistrate works only one shift per day. Within these constraints, magistrates are randomly assigned to different shifts. With each of the false work schedules I regress each covariate on fixed effects for the magistrate predicted to work under the false schedule, again controlling for the time and date of the bail hearing. I collect and store the F statistic from the false-schedule magistrate fixed effects and use these to build an empirical distribution of the F statistic under five-day-per-shift work schedules. This F distribution represents the range of F values that are likely to be seen under the block randomization that characterizes the magistrates' work schedules. I then compare the F statistic from the real data to the empirical distribution of the F statistic. The fraction of false-schedule F statistics which are greater than the real F statistic is the empirical p-value. The distribution of the empirical p-values is shown in Figure 1A. They are evenly distributed between 0 and 1; if anything, the F statistics from the false-schedules are slightly larger, on average, than the F statistic from the real data.

Table 1A shows the real-data F statistics next to the empirical p-values for three summary statistics: the predicted likelihood of pretrial detention, the predicted likelihood of pleading guilty, and the predicted likelihood of conviction. Each predicted likelihood is the fitted value from a regression of detention, pleading guilty, and conviction (respectively) on all of the rest of the covariates and time controls. In essence, they are a weighted average of the covariates that most strongly predict each outcome. The empirical p-values suggest that there is no strategic behavior by defendants hoping to receive a lenient magistrate.

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[34]The process is computationally expensive across which is why I only build 500 false-schedules.

Figure 1A: Empirical distribution of p-values in permuted randomization test
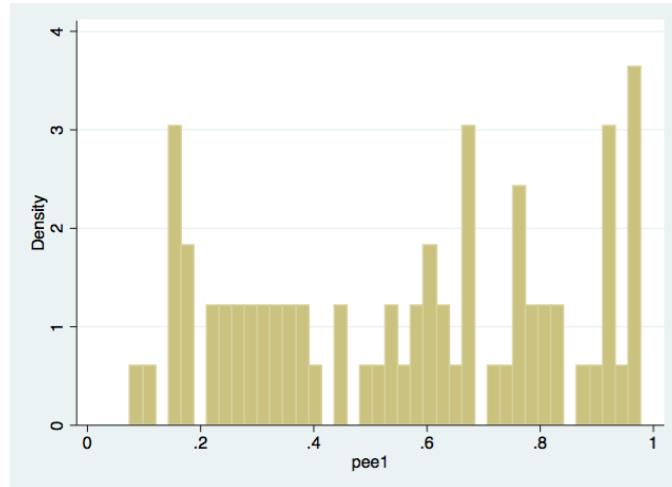
Note: This figure shows a histogram of 'empirical p-values' from a permutation test. The permutation test involves regressing various covariates on magistrate dummies under false work schedules. The F statistic on the magistrate dummies using the real data is compared to the distribution of F statistics on false-schedule magistrate dummies. The empirical p-value is the fraction of the false-schedule F statistics that are greater than the true-schedule F statistic. The results for 70 covariates plus three summary statistics (the predicted likelihood of being detained, convicted and pleading guilty) are shown.

Table 1A: Permutation test for randomization

Summary statistics for defendant characteristics	F statistic	Empirical p-value
Predicted likelihood of pretrial detention	2.50	0.670
Predicted likelihood of pleading guilty	3.29	0.267
Predicted likelihood of conviction	2.14	0.554

Note: The dependent variables in the left column are the predicted values from a regression of pretrial detention, pleading guilty, and conviction, respectively, on offense, criminal history, demographics and time controls. The middle column shows the F statistics in a test of joint significance of eight magistrate dummies. Controls for the time and date of the bail hearing are included in each regression. The numerator and denominator degrees of freedom are 7 and 331,946 respectively. The empirical p-values are the fraction of 'false' F statistics larger than the true F statistic in a permutation test.

Table 1B: Alternative specifications

Panel A: The impacts of the bail amount on case outcomes (IV)

	(1) Conviction	(2) Guilty plea	(3) Court Fees	(4) Incarceration	(5) Max days	(6) Min days
Bail (in thousands)	0.000532*	0.000368	0.879**	0.000457*	-0.242	-0.319
	(0.000281)	(0.000234)	(0.373)	(0.000254)	(1.916)	(1.775)
Observations	331971	331971	331971	331971	331971	331971

Panel B: The impacts of non-monetary release on case outcomes (IV)

	(1) Conviction	(2) Guilty plea	(3) Court Fees	(4) Incarceration	(5) Max days	(6) Min days
Non-financial release	-0.0118	-0.00394	-113.1****	-0.00408	38.34	17.82
	(0.0148)	(0.0131)	(19.72)	(0.0131)	(26.36)	(21.04)
Observations	331971	331971	331971	331971	331971	331971

Panel C: Controlling for phone call-ins (IV)

	(1) Conviction	(2) Guilty plea	(3) Court Fees	(4) Incarceration	(5) Max days	(6) Min days
Phone call-ins	-0.00840	0.0109	-18.41*	-0.0163**	5.470	21.35
	(0.0114)	(0.00949)	(10.78)	(0.00828)	(17.61)	(14.49)
Pretrial detention	0.0524	0.0594**	108.5***	-0.0000721	131.0	160.8**
	(0.0325)	(0.0291)	(37.51)	(0.0276)	(86.19)	(72.40)
Observations	331971	331971	331971	331971	331971	331971

Standard errors in parentheses

Heteroskedastic-Robust Standard Errors

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: Panel A estimates the impact of the bail amount (in thousands of dollars) on case outcomes. Panel B estimates the impact of non-financial release on case outcomes. Panel C instruments for two endogenous variables: pretrial detention and a dummy which is equal to one if the defendant is given a non-monetary release with the condition of needing to make weekly or bi-weekly phone calls to an automated voice system. The outcome variables are dummies for being convicted/pleading guilty, total non-bail court fees in dollars, receiving an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. In all specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history and demographics of defendants. All regressions include the full set of controls as described in Section 5.

Table 1C: Comparing results across defendant race

	(1) Conv- iction	(2) Guilty plea	(3) Court Fees	(4) Incarc- eration	(5) Max days	(6) Min days
Panel A: White defendants (IV)						
Pretrial detention	0.0802	0.0223	88.64	-0.0285	195.8	236.4**
	(0.0590)	(0.0549)	(75.38)	(0.0532)	(135.4)	(109.8)
Panel B: White defendants (OLS with controls)						
Pretrial detention	0.0355****	0.0505****	-115.9****	0.102****	147.1****	74.99****
	(0.00371)	(0.00351)	(5.315)	(0.00334)	(6.037)	(4.019)
Observations	94076	94076	94076	94076	94076	94076
Mean dep. var.	0.55	0.29	$361	0.27	254	124
Panel C: Black defendants (IV)						
Pretrial detention	0.0664*	0.0204	113.8***	-0.00911	53.83	107.0
	(0.0392)	(0.0353)	(44.05)	(0.0338)	(112.6)	(95.66)
Panel D: Black defendants (OLS with controls)						
Pretrial detention	0.0393****	0.0599****	-99.68****	0.0964****	132.0****	66.45****
	(0.00258)	(0.00234)	(3.337)	(0.00216)	(4.718)	(3.520)
Observations	191379	191379	191379	191379	191378	191378
Mean dep. var.	0.49	0.25	$296	0.25	357	196

Standard errors in parentheses

Heteroskedastic-Robust Standard Errors

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: This table shows effect sizes among white defendants (Panels A and B) and black defendants (Panels C and D). The outcome variables are dummies for being convicted/pleading guilty, total non-bail court fees in dollars, a dummy for whether or not the defendant receives an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. In all of the IV specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history and demographics of defendants.

Table 1D: Comparing results across defendant age

	(1) Conv- iction	(2) Guilty plea	(3) Court Fees	(4) Incarc- eration	(5) Max days	(6) Min days
Panel A: Defendants under 30 (IV)						
Pretrial detention	0.0359	0.0608	82.73	-0.00439	264.3	245.3
	(0.0636)	(0.0578)	(76.43)	(0.0556)	(209.4)	(182.4)
Panel B: Defendants under 30 (OLS with controls)						
Pretrial detention	0.0362****	0.0551****	-120.3****	0.0990****	139.4****	69.39****
	(0.00281)	(0.00259)	(3.817)	(0.00239)	(5.401)	(4.155)
Observations	167586	167586	167586	167586	167585	167585
Mean dep. var.	0.47	0.27	$304	0.24	348	193
Panel C: Defendants over 30 (IV)						
Pretrial detention	0.0716**	0.0521	179.0****	0.0217	28.99	57.72
	(0.0358)	(0.0324)	(40.79)	(0.0306)	(76.78)	(62.86)
Panel D: Defendants over 30 (OLS with controls)						
Pretrial detention	0.0322****	0.0585****	-84.30****	0.0948****	126.6****	67.27****
	(0.00278)	(0.00254)	(3.600)	(0.00233)	(4.272)	(2.833)
Observations	164356	164356	164356	164356	164355	164355
Mean dep. var.	0.51	0.25	$320	0.24	235	117

Standard errors in parentheses

Heteroskedastic-Robust Standard Errors

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: This table shows effect sizes among defendants under 30 (Panels A and B) and defendants over 30 (Panels C and D). The outcome variables are dummies for being convicted/pleading guilty, total non-bail court fees in dollars, a dummy for whether or not the defendant receives an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. In all of the IV specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history and demographics of defendants.

Table 1E: Comparing results across defendants' history of arrest

	(1) Conv- iction	(2) Guilty plea	(3) Court Fees	(4) Incarc- eration	(5) Max days	(6) Min days
Panel A: Defendants with zero or one prior arrests (IV)						
Pretrial detention	0.118	0.0916	40.44	-0.123**	169.7	245.2
	(0.0788)	(0.0727)	(105.1)	(0.0624)	(213.0)	(173.5)
Panel B: Defendants with zero or one prior arrests (OLS with controls)						
Pretrial detention	0.0192****	0.0470****	-103.9****	0.0837****	136.5****	72.49****
	(0.00338)	(0.00311)	(4.637)	(0.00259)	(5.494)	(3.896)
Observations	124344	124344	124344	124344	124342	124342
Mean dep. var.	0.42	0.24	$320	0.17	200	107
Panel F: Defendants with two or more prior arrests (IV)						
Pretrial detention	0.0625**	0.0445	151.8****	0.0721**	183.6**	181.8***
	(0.0317)	(0.0284)	(35.15)	(0.0284)	(78.48)	(66.94)
Panel G: Defendants with two or more prior arrests (OLS with controls)						
Pretrial detention	0.0450****	0.0551****	-100.8****	0.101****	131.9****	66.81****
	(0.00244)	(0.00224)	(3.217)	(0.00216)	(4.302)	(3.145)
Observations	207627	207627	207627	207627	207627	207627
Mean dep. var.	0.53	0.27	$307	0.28	347	184

Standard errors in parentheses

Heteroskedastic-Robust Standard Errors

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Note: This table shows effect sizes among defendants with zero or one prior arrests (Panels A and B) and defendants with two or more prior arrests (Panels C and D). The outcome variables are dummies for being convicted/pleading guilty, total non-bail court fees in dollars, a dummy for whether or not the defendant receives an incarceration sentence, the maximum days of that incarceration sentence and the minimum days the defendant must serve before being eligible for parole. In all of the IV specifications magistrate preferences are allowed to vary across three time periods and according to offense, criminal history and demographics of defendants.